

The Impact of Juvenile Curfew Laws*

Patrick Kline
UC Berkeley / NBER
pkline@econ.berkeley.edu

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Abstract: Youth curfew ordinances are a widely touted, yet little studied policy tool available to local police departments. This paper evaluates the effectiveness of curfew ordinances by comparing the arrest behavior of various age groups within a city before and after curfew enactment. The evidence suggests that curfews are effective at reducing both violent and property crimes committed by juveniles below the statutory curfew age. Curfews do not appear to be effective at influencing the criminal behavior of youth just above the curfew age, suggesting that the choice of statutory curfew age is important in crafting policy.

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“[Youth curfews] help keep our children out of harm’s way. They give parents a tool to impart discipline, respect, and rules at an awkward and difficult time in children’s lives.”

- Bill Clinton (1996)

“Can you tell the difference between a 19-year-old (who may be exempt from a curfew) and a 17-year-old (who may not be)? A law that gives the police the right -- indeed, requires them -- to stop people on the basis of their perceived age is an invitation to trouble.”

- Geoffrey Canada (1996), president of Rheedlen Centers for Children and Families

I. Introduction

In the early 1990’s youth curfews became a popular strategy for combating juvenile delinquency. A survey by Ruefle and Reynolds (1996a) found that 146 of the 200 American cities with population above 100,000 had curfew laws on the books, with 110 having enacted or revised curfew laws between 1990 and 1995. A subsequent study by the U.S. Conference of Mayors (1997) found that 80% of the 347 cities with population over 30,000 had youth curfew ordinances.

Despite their popularity with local governments, little is known about the effects of curfew laws on youth outcomes. While some research attempts to estimate the effect of curfews on violent crimes committed by and perpetrated against youths, the findings rely on tenuous identifying assumptions.¹ Furthermore, existing studies have ignored one of the central questions involving curfews, which is whether they have spillover effects on other age groups.

This paper evaluates the effectiveness of curfew ordinances by comparing the arrest behavior of various age groups within a city before and after curfew enactment. Since curfew

¹ See Adams (2003) for a review of the literature.

ordinances only apply to youth of or below a given age, usually 16 or 17, young people just above a city's statutory maximum curfew age provide a natural control group. However, there are reasons to believe that curfew laws may affect these exempt age-groups as well. By comparing outcomes for age-groups several years older than the statutory maximum age to those just above the maximum age, I am able to test for the presence of any such spillover effects.

Analyzing both sets of comparisons is important because curfew policies can be thought of as constituting two treatments, each applying to a different set of age-groups. The first treatment, the *statutory* treatment, is that of being subject to a curfew citation, fine, temporary detention, or whatever punishment is statutorily prescribed for curfew violations by minors. This treatment only affects those youth under the statutory curfew age. The second treatment, the *statistical discrimination* treatment, is that of being subject to lower standards of probable cause because of one's perceived youth. Police are unlikely to be able to distinguish between young people just above and below the curfew age. Thus, for adjacent age groups curfews should raise the probability of being stopped or searched by an amount that depends very little on one's actual age. The possibility of this second effect is frequently cited by the ACLU as an argument for reversing such ordinances on the grounds that they constitute violations of fundamental civil liberties.

Constitutional issues aside, both treatments should be of interest to economists. The statutory treatment represents the deterrent effect of the curfew's statutory sanctions. Identifying this effect tells us how much crime could be reduced by raising penalties or increasing

enforcement of curfew ordinances.² It also implicitly provides an estimate of an important margin previously unexamined in the economics of crime, the substitutability of criminal activity across time.³ Indeed, if, as in basic economic models (Becker, 1968), crime is a purposive activity, then curfews should only deter delinquency if the technology used to produce this behavior is imperfectly substitutable between curfew and non-curfew hours.⁴ Borrowing from the optimal taxation literature, if the degree of substitutability is very low, then “taxing” nocturnal youth activity may be a viable “second-best” solution to the problem of delinquency.

The statistical discrimination treatment tells us the impact of weakening 4th Amendment protections against unreasonable search and seizure. It is well understood by economists and legal theorists that rights impose costs and benefits, the distribution of which may not lead to efficiency. Estimates of this margin are important not only for those interested in evaluating the costs and benefits of the age discrimination implicit in youth curfews, but those involved in the recent debate over racial profiling and national security. Furthermore, these estimates are closely tied to the elasticity of criminal behavior with respect to the probability of detection, a key parameter in Becker’s classic model.⁵

Under the assumption that police cannot distinguish between adjacent ages ex-ante, comparisons of the response of age groups just below the curfew age to those just above will

² As such, the paper falls into the well developed economics literature on optimal fines. See Becker (1968), Stigler (1970), Polinsky and Shavell (1984) for theory and Bar-Ilan and Sacerdote (2004) for an example of recent empirical work.

³ See Jacob et al. (2004) for an examination of the substitutability of criminal activity across days as opposed to within a day.

⁴ Note that even if youth crime is largely spontaneous and unplanned, we may still find that curfews have an effect by reducing the preconditions (underage drinking, loitering at night) that support such impulsive behavior.

⁵ I will not be estimating this parameter directly since I am measuring the response of arrests to the statistical discrimination treatment rather than an index of criminal activity itself. However, given a prior on the amount by which the probability of detection is raised, a posterior on Becker’s elasticity can be inferred from the estimates.

estimate the statutory treatment effect. Similarly, statistical discrimination effects can be estimated by comparing the response of age-groups just above the curfew age to those several years older. There is, however, one important caveat to this interpretation which is that the behavior of various age groups in a city may be dependent due to cross-age social interactions. Such interactions may yield second order effects of the statutory treatment on age-groups just above the curfew age. To the extent that the behavior in question exhibits positive dependence across ages (i.e. decreases in the behavior among one age-group lead to decreases in that same behavior among adjacent age-groups) the statutory estimates will be biased towards zero. Conversely, negative dependence in outcomes will bias statutory estimates away from zero. Second order effects will also yield biases in estimates of statistical discrimination effects, with the direction of bias depending on the nature of the dependence and the sign of the effect.⁶

Thus, making inferences about the mechanism through which curfews affect relative outcomes requires placing bounds on the sign and magnitude of these second order effects. Rather than engaging in a formal bounding analysis, I discuss the results in a reduced form manner, while suggesting the role that second order effects might be playing for each outcome. A reasonable assumption, however, is that any second order effects are smaller than the first order effects which generated them. This implies that estimates of statutory treatment effects will have the correct sign regardless of the nature of the dependence. Statistical discrimination estimates on the other hand may yield the incorrect sign if statutory effects are large and second order effects are important, which is why I refer to the reduced form estimates as “spillover” effects.

⁶ For instance if both the statutory and statistical discrimination effects are negative and there is negative dependence, the statutory estimates will be biased away from zero, while the statistical discrimination estimates will be biased towards zero.

To preview the results, I find strong evidence of persistent statutory treatment effects on criminal behavior. Arrests for violent crime and property crime both appear to fall by around 10% in the year after curfew enactment, with the effects on violent crime intensifying substantially in subsequent years. No evidence is found of important spillovers effects on either measure of arrests.

Section II provides background on curfew legislation, Section III describes the econometric methodology, Section IV describes the data, Section V provides results, Section VI concludes.

II. A Brief Introduction to Youth Curfew Laws

Juvenile curfews are local ordinances proscribing minors, generally within a specified age range, from occupying public areas and streets during particular times. These policies are not new. The first youth curfew was enacted in Omaha, Nebraska in 1880 (Hemmens and Bennett, 1999). In 1884, President Harrison gave a speech endorsing curfews as “the most important municipal regulation for the protection of the children of American homes, from the vices of the street” (Note, 1958). By 1957, 57 of the 109 cities with 1950 population over 100,000 reported having curfews (Note, 1958).

Although many cities have long had curfew statutes, the most recent period of sustained enforcement came in the early nineties when violent crime and victimization of juveniles began to rise and cities learned how to craft curfew legislation in a manner amenable to the courts. Previous to this period, many cities had been unable to defend their curfews from legal

challenges that they violate civil rights, especially the first, fourth, and fourteenth amendments of the constitution.

Many cities, fearful of challenges by the ACLU and others, either allowed enforcement of their laws to lapse or in some cases actually repealed existing ordinances. It is against this backdrop that Dallas, Texas passed a new curfew ordinance in 1991 that would become a model for many other American cities. The Dallas curfew was narrowly tailored to apply to youth of specified ages, at specific times, and had a number of exemptions to the law including for youth accompanied by an adult, responding to an emergency, and traveling to or from school, work, or a religious service. Furthermore, the parameters of the curfew were designed to deal with the specific needs of the city. The city had collected data showing, among other things, that juvenile delinquency increased proportionally with age between the ages of 10 and 16, that the time during which murders by juveniles were most frequently committed was between 10 p.m. and 1 a.m., that these murders most often occurred in apartments, parking lots, and streets and highways, that aggravated assaults by juveniles were most likely to occur between 11 p.m. and 1 a.m., and that 31 percent of robberies occurred on public streets and highways (Department of Justice, 1996).

The ACLU challenged Dallas's law shortly after it was introduced, causing a judge to issue an injunction against its immediate enforcement. In *Quib v. Strauss* (1993) the U.S. Court of Appeals for the Fifth Circuit upheld the law, arguing that the city demonstrated that the ordinance was sufficiently narrowly tailored and that it met a compelling state interest, the two conditions necessary for passing the "strict scrutiny" test of constitutional infringement. An appeal was made to the Supreme Court which refused to hear the case, thus setting a precedent

for the design of youth ordinances. Even before the Fifth Circuit upheld the law, local governments were paying close attention to the construction of the Dallas curfew. Cities such as Miami, FL, El Paso, TX, and San Antonio, TX explicitly modeled their programs on Dallas's. By 1996, President Clinton was publicly touting youth curfews as an effective policy for combating juvenile delinquency and the Department of Justice and U.S. Conference of Mayors were issuing briefings to local governments on best practices for curfew creation (U.S. Department of Justice, 1996; U.S. Conference of Mayors, 1997).

The Dallas Model

Although the specifics of curfew implementation vary by city, it is worth delving into the details of the Dallas program so that we might understand the issues involved. The Dallas curfew applied to all youth under the age of 17 and proscribed them from being in public places during the hours of 11 pm – 6 a.m. on weekdays and 12 a.m. – 6 a.m. on weekends. Before the curfew was implemented on May 1, 1994, the Dallas Police Department put out public service announcements in English and Spanish on the radio and in poster form to announce that the ordinance would soon be enforced. They also held a well covered press conference explaining details of the law. Furthermore, a week before the curfew was implemented police handed out warning fliers to youth in public during curfew hours.

Once the curfew actually went into effect, police had substantial discretion over how they would implement the ordinance. Police could give youths in violation of the curfew a verbal warning, take them home, issue a ticket with a fine as high as \$500, or take them into custody. A youth detention facility was staffed by the city for holding curfew violators. If a child was found

in repeated violation of the curfew, police had the authority to fine the child's parents up to \$500. Furthermore, businesses could also be fined for allowing minors to remain on their premises during curfew hours. In conjunction with these penalties were a series of youth programs including a midnight basketball program and a youth education program. Other cities such as New Orleans went further than this and sent minors picked up on curfew violations to a detention center staffed by psychologists, medical professionals, and clergy to provide counseling for violators.

In the first 3 months of the Dallas program no arrests were made for curfew violations, but hundreds of warnings and citations were handed out to youth and 8 tickets were written to adults for permitting violations. In an interim review of the program, the Dallas police department found that juvenile victimization during curfew hours had dropped 17.7% from 1,950 during the period from May to July 1993, to 1,604 during the same period in 1994. Considering that no youths were actually arrested for curfew violations during this period, this result may seem surprising. However, as made clear above, arrests are only one tool made available to police officers by the curfew. The greatest treatment induced by the curfew may be the weakened standards of probable cause. As one Dallas police officer put it:

"There's no way I'm going to stop every kid I see... I come down on them when I suspect they're into something else, like breaking into a car or vandalizing. When I stop them for those offenses, the curfew gives me an extra tool of enforcement. If they're not guilty of the offense I suspected them of, an underage (16 or under) person can still be hit with the curfew." (Bell, 1994)

But clearly, even if the youth is not underage, curfew laws provide police with a legal justification for stopping and questioning people who appear to be young. This has the potential to lead to the arrests of many more young people for serious crimes than would otherwise have been apprehended by the police.

III. Methodology

To date there has been little empirical work evaluating youth curfew laws. The statistical studies that do exist typically involve identifying treatment effects off of the differential timing of the adoption of curfew laws by city (McDowell et al., 2000; Males and Macallair, 1999). This approach implicitly assumes that cities that have not yet adopted a curfew law are a good control group for cities that have. But if we consider the process generating the differences in the timing of curfew adoption, we should expect that cities adopt these laws in response to increases in violent crime by and victimization of juveniles. If these shocks are persistent, the treatment variable is likely to be correlated not only with past, but current and future values of the error term, a violation of the standard exogeneity requirements for identification.

To illustrate the problem, suppose that curfews are enacted one year after a permanent increase in the growth rate of a city's level of juvenile crime.⁷ Say also that the timing of these changes is randomly distributed across cities. Then if we compare the change in crime in a city a year after it has implemented its curfew to all other cities that have not yet enacted their curfew in that same year, the difference in means will contain the "true" treatment effect, plus some fraction of the trend change that motivated the treatment, the fraction depending upon how many of the control cities have not yet had their crime growth rate increase. Since the true treatment is presumably negative, we should expect that results from simple first differenced regressions with time effects will lead to estimates biased up towards zero. For this reason it is not surprising that past studies have typically failed to find an effect of curfews on juvenile crime.

⁷ This example is somewhat unrealistic since the secular trends leading to the differential adoption of curfew ordinances are probably not permanent changes in growth rates, but rather shocks that die out over time. Nevertheless, the same logic can be used to show that first differenced estimates will be biased towards zero when the treatment is correlated with lagged errors which are autocorrelated.

Another problem with previous research is that little attention has been paid to the mechanisms through which curfews might affect youth outcomes. As described in the introduction, curfews are likely not only to affect those youths legally subject to the curfew because of their age, but also those who are above curfew age but look as if they could be subject to the ordinance. For an evaluation to provide information about the deterrent properties of the punishments associated with curfew violation itself, one must disaggregate the overall treatment effect into its constituent parts.

This can be accomplished by exploiting differences in the target population of each sub-treatment. The statutory treatment only affects young people below curfew age, while the statistical discrimination treatment affects all young people. Thus, by comparing people just below and just above the curfew age, one can infer the statutory treatment effect. Similarly, by comparing young people just above the curfew age with young adults, it should be possible to estimate any statistical discrimination effects. The identifying assumption in this case is that, conditional on the controls, the factors influencing both age groups are identical except for the sub-treatment in question.

Modeling Outcomes

Consider the following econometric model of arrests:

$$(1) y_{cay} = \sum_t \beta_t D_{cay}^t + \phi_{cy} + \psi_{ay} + \theta_{ca} + \varepsilon_{cay}$$

where y_{cay} is the log of the number of arrests in city c , among individuals of age-group a , in calendar year y , ϕ_{cy} is a vector of city-year effects, ψ_{ay} is a vector of age-group/year effects, θ_{ca} is a vector of city/age-group means, and ε_{cay} is an error term which may exhibit arbitrary dependence within city but is uncorrelated with the other right hand side variables. D_{cay}^t is a dummy variable that equals one when curfew enactment is t periods away for a given age group in a city;⁸ it equals zero in all periods for age groups never subject to a curfew in a given city.⁹ Thus, the β_t coefficients represent the time path of outcomes relative to the date of curfew enactment for age-groups subject to the curfew conditional on the four unobserved variance components ϕ_{cy} , ψ_{ay} , θ_{ca} , and ε_{cay} . If curfews are randomly assigned the following restriction should hold:

$$(1a) \beta_t = 0 \quad \forall t < 0$$

In words, this means that, prior to enactment, groups subject to curfew laws should exhibit time series behavior that is on average identical to groups not subject to curfews.

The variance components model in (1) is fairly flexible, allowing for city and age specific year shocks to outcomes along with different means for each city/age-group combination. A key assumption is that the city-year effects ϕ_{cy} , the age-group/year effects ψ_{ay} , and the city-age intercepts θ_{ca} are additively separable. Substantively this implies two important conditions. One

⁸ I will often refer to the city/age-groups subject to the curfew as the “treatment group” and the city/age-groups not subject to the curfew as the “control group”. The composition of these groups varies, depending upon the outcome and the effect being studied. See Tables 3 and 4.

⁹ i.e. $D_{cay}^t = I[y - e_{ca} = t]$ where $I[.]$ is an indicator function and e_{ca} is the year a curfew is enacted in city c for age-group a . For age-groups never subject to a curfew in a city, e_{ca} is undefined and D_{cay}^t evaluates to zero.

is that there are no unobserved time varying factors that differentially affect age groups within a city. The second is that the scale of the city/year effects does not depend on the level of the city/age-group means. The former assumption can be tested empirically by examining the pre-enactment behavior of different age groups within a city. The latter is a functional form assumption that is reasonable given a logarithmic transformation of the dependent variable so that all unobserved effects are scale independent.

Were the error term ε_{cay} approximately normal and homoscedastic, equation (1) could be well estimated via OLS. However, inspection of the data suggests that these conditions are substantially violated in practice. The age specific arrest data found in the Uniform Crime Reports is plagued with reporting errors and outliers, yielding a skewed distribution of log arrests with very fat tails. Rather than attempt to manually trim outliers, I use median regression as a robust estimator of location. To avoid the problems inherent in estimating the nuisance parameters in (1), I eliminate the city-age and city-year fixed effects by differencing the data across time and age groups. First differencing over time has the added advantage of substantially reducing the serial correlation in the error term.

Define the operator Δ_a as a function taking the difference between adjacent age values of a variable (i.e. for any variable x_{cay} , $\Delta_a x_{cay} = x_{cay} - x_{ca'y}$ where $a' = a - 1$).¹⁰ Similarly, let the operator Δ_y take the difference between adjacent year values of a variable. Since the dependent variable y_{cay} is in logs, the difference $\Delta_a y_{cay}$ can be thought of as the log of the ratio of outcomes between age groups. For each city enacting a curfew, the dataset includes a single age-

¹⁰ And for any variable x_{cy} , $\Delta_a x_{cy} = x_{cy} - x_{cy} = 0$.

group above the curfew age and a single age-group just below the curfew age. Thus, each of the cities enacting a curfew contains a single ratio $\Delta_a y_{cay}$ for which $\Delta_a D_{cay}^t = 1$. The dataset also includes a group of cities which never enacted curfews.¹¹ For cities not enacting curfews, three ratios are included in the dataset corresponding to ages 15, 16, and 17, each ratio having $\Delta_a D_{cay}^t = 0$. Thus, given the structure of the data, $\Delta_a D_{cay}^t$ is equivalent to a dummy variable E_{cy}^t that equals one when a city is t periods away from enacting a curfew and always equals zero for control cities.

Using this notation we can write the transformed equation:¹²

$$\begin{aligned} (2) \quad \Delta_y \Delta_a y_{cay} &= \sum_t \beta_t \Delta_y \Delta_a D_{cay}^t + \Delta_y \Delta_a \phi_{cy} + \Delta_y \Delta_a \psi_{ay} + \Delta_y \Delta_a \theta_{ca} + \Delta_y \Delta_a \varepsilon_{cay} \\ &= \sum_t \beta_t \Delta_y E_{cy}^t + \Delta_y \Delta_a \psi_{ay} + \Delta_y \Delta_a \varepsilon_{cay} \end{aligned}$$

The reader familiar with such models will recognize that $\sum_t \Delta_y E_{cy}^t = 0$. Thus, not all of the β_t 's can be identified because the E_{cy}^t 's are perfectly collinear even in the absence of a constant.¹³ For this reason I normalize $\beta_{-1} = 0$, so that all post-enactment coefficients can be thought of as treatment effects. To ensure that the coefficients are well estimated, I restrict the sample to 6

¹¹ These cities are included to alleviate the near-multicollinearity of the estimated variance components. All of the models estimated in this paper are still identified (though relatively inefficient) without them.

¹² Note that this transformation is the familiar "difference in difference" of the dependent variable.

¹³ The same is true of the ψ_{ay} 's, although imposing a normalization on these parameters does not affect our estimates of the β_t 's

years of data before and after curfew enactment for enacting cities. Block bootstrap methods are used for hypothesis testing.¹⁴

IV. Data

The data on curfew enactment were collected from a variety of sources.¹⁵ I start with the list given in Ruefle and Reynolds (1996b), who surveyed the universe of cities with a 1992 population of 100,000 or more. They queried each city's police department as to whether a youth curfew had been enacted, the hours of the curfew, whether the curfew was newly enacted or revised, and which age-groups were subject to the curfew. Comparisons of the data with city codes and newspaper stories indicated that some of the information was inaccurate. Many cities had enacted curfews prior to the dates listed in the survey and some had not enacted curfews at all. Furthermore, some of the information on which age groups the curfew applied to was incorrect.

To deal with these data quality problems I acquired municipal codes from all 92 cities with a 1990 population greater than 180,000.¹⁶ These codes generally contained a history of revisions and a description of exactly which age groups were covered by the curfew ordinance. In some cases the revision history was not listed and I contacted city clerks directly to inquire about previous ordinances and prior versions of the code. To be sure that the code did not refer to a rewritten version of an old ordinance, I searched the ProQuest and Lexis-Nexis periodical

¹⁴ See Horowitz (2001) for a review of bootstrap methods. In all cases I use the block bootstrap clustered at the city level to calculate p-values for the null hypothesis that the moments of interest equal zero. The bootstrap draws are stratified by statutory curfew age and whether a city ever enacts a curfew in order to ensure that suitable control cities exist in each bootstrap draw.

¹⁵ See the Data Appendix for a detailed description of the construction of the data.

¹⁶ This number was chosen for convenience. It became increasingly difficult to obtain reliable contact information, newspaper stories, and records as city size decreased.

indices for newspaper stories detailing the process of curfew enactment in each city for 5 years before and after each of the suspected dates of enactment. Using this information I compiled a legislative history of each city's curfew law.

After cross-referencing the legislative history, the list from Ruefle and Reynolds, and newspaper articles, I selected 65 cities for use in the empirical work.¹⁷ Of the 65 cities, 54 enacted curfews during the late eighties and early 90's, while 11 cities never enacted curfews and were included as controls for purposes of estimating the age-group/year effects. Table 1 shows the final list of cities retained in the analysis and their associated curfew information. There is no obvious pattern as to which cities chose to (or not to) enact curfews. Cities of all sizes and in all regions made use of such ordinances.

I use the FBI's Unified Criminal Reporting (UCR) files to obtain detailed information on arrests by age, city, gender, and type of offense for the years 1984-2002. Some cities do not report information for all offenses in all years, and some have values in some years which are clearly erroneous.¹⁸ I drop only the most serious cases of erroneous data from my analysis of arrests, relying on the median regression to automatically reduce the influence of any further outliers. Because the reporting quality of female arrest data is lower, I only examine arrests of men.¹⁹

Approximately 30% of the Uniform Crime Reporting data on arrests is missing. Note however that if either age group (treatment or control) is missing data the dependent variable

¹⁷ The details of the method used to select cities and assign enactment dates are explained in the appendix.

¹⁸ San Antonio for instance, lists 727 arrests of 17-year olds for violent crime in 1992, but only 7 in 1993.

¹⁹ Arrests for women are about 1/7th the level of men and it is more common for cities not to report arrest data on women to UCR.

$\Delta_y \Delta_a y_{city}$ will be missing as well. Thus, as long as a city's decision to report arrest data is not a function of the differential change in outcomes for a specific age-group, the missing data will not induce bias. Since cities tend to report data on all ages when they report at all, it seems unlikely that the decision to report is a function of age-specific fluctuations in arrests.

Arrests are not a perfect measure of youth criminal behavior, since they also reflect the behavior of police. However, detailed age data is not available in UCR offense reports and other work indicates that arrest data provide fairly accurate representations of underlying criminal activity.²⁰ To deal with issues of police discretion I focus on serious felonies which are unlikely to be reclassified as curfew violations. Thus, we should expect that if curfew laws change the behavior of police, they should allow them to more easily apprehend and arrest minors below the curfew age (to the extent that such minors are capable of being distinguished from their peers), biasing estimates of the statutory treatment effect up towards zero.

Table 2 shows summary statistics for all outcomes by age-group.²¹ The "Violent Crimes" variable is an unweighted sum of the following offenses: murder, manslaughter, rape, robbery, aggravated assault. The Property Crimes variable is the sum of the remaining FBI Type I offenses: burglary, larceny, motor vehicle theft, and arson. Burglary and larceny constitute a large fraction of the property crime index, while assaults make up the bulk of the violent crime index. Very few observations are lost when taking the log.

V. Results

²⁰ See Cook and Laub (1998).

²¹ See Tables 3 and 4 for exact definitions of the various age-groups.

Statutory Treatment Effects

Figure 1a plots the estimated β_t coefficients from a median regression of the form given in (2) where the dependent variable is log arrests for violent crime. The bands around the point estimates are 90% bootstrapped confidence intervals.²² The treated age-group is the oldest single digit age subject to the curfew while the control age group consists of youth exactly one year older.

The story told by the figure is rather striking. Prior to enactment there is a slight upward trend in the relative number of arrests of youth below the curfew age, but starting in the year of enactment this pattern changes abruptly with arrests falling by 8% contemporaneously and then further plummeting in subsequent years to levels as much as 30% below pre-treatment values.

Figure 1b shows analogous estimates for property crime arrests. Property crimes exhibit much the same behavior as violent crimes with what appears to be an initial 8% reduction in arrests in the year of enactment and further reductions in subsequent years culminating in an estimated 17% reduction in arrests after 6 years. However, while the contemporaneous treatment effect can be statistically distinguished from zero, the other coefficients cannot.

Although the general pattern of the figures is clear, the individual β_t coefficients are quite imprecise. Table 3 provides more formal tests of the null hypotheses that curfews are ineffective. In order to gain statistical power I test hypotheses about averages of the β_t coefficients over various time intervals and experiment with reducing the parameterization of the

²² These confidence intervals are robust to arbitrary within-city correlation in the errors.

model. Every other row of Table 3 imposes condition (1a),²³ which effectively measures all treatment effects relative to a pre-treatment average level of the dependent variable instead of the period immediately prior to enactment. For convenience OLS estimates are shown as well, though the median regression (LAD) estimates are preferred for efficiency reasons.

The results of the Table are in keeping with the pattern suggested by the figures. The estimated average reduction in violent crime due to curfew enactment in the three years following enactment is 20% and statistically distinguishable from zero. The average effect over the seven years starting with the year of enactment is 23% which is also distinguishable from zero. Imposing condition (1a) has little effect on the violent crime point estimates but substantially reduces their variability.

The estimated average reduction in property crime arrests over the 3 years following enactment is 9%, while the average effect over the entire 7 year sample period is 12%. Neither of these estimates is distinguishable from zero in the fully parameterized model. Imposing condition (1a) however makes all of the estimates significant at the 10% level.

For both outcomes, the estimated long run effects may be influenced by the tendency of cities to re-write and reenact their curfew ordinances. Collecting information on dates of reenactment is difficult since changes in the enforcement of existing ordinances may not have been documented by the newspapers or changes in the city code. Furthermore, it is not clear how best to use such information even if it were available. Dropping cities that reenact curfews would presumably yield biased estimates of treatment effects since reenactment is often a response to the perceived

²³ This restriction cannot be rejected by the data.

effectiveness of the existing ordinance. Thus, rather than attempt to infer what would have happened if no city in the sample modified its existing curfew, I confine myself to analysis of what actually did happen relative to the counterfactual of no curfew ordinance having ever been enacted.

The upshot of this discussion is that when interpreting the dynamic effects in Table 3 and the figures, it is important to remember that the data cannot distinguish between lags in the behavior of youth, lags in the enforcement behavior of police, and lags in the legal environment of cities. It could be that the effects are intensifying because the cities tend to reenact or expand the legal status of curfews over time, because police slowly learn how best to use curfew legislation to their advantage, or because youth are only slowly deterred. Moreover, all three could well be true.

Spillover Effects

The previous section documented the differential impact of curfews on age-groups just of or below curfew age relative to age-groups just above curfew age. This section considers whether age-groups just above curfew age are also affected by curfews. This could occur for two reasons. One is that law enforcement officials cannot infer the age of individuals and thus question and search individuals who look young regardless of whether such people are actually legally subject to the curfew. Another is that an age-group's propensity to commit crimes may be a function of the number of crimes committed by other age-groups.²⁴

²⁴ Levitt (1998) finds that changes in the severity of penalties for crimes committed by adults do not appear to affect the propensity of juveniles to commit crimes or vice versa, suggesting that any such cross-age dependence is probably minimal.

Figures 2a-2b plot comparisons of arrests of youth just above curfew age against a new control group of somewhat older youth.²⁵ Exact definitions of the treatment and control groups, point estimates of treatment effects, and p-values for statistical tests are given in Table 4. Since the treatment and control groups now differ in age by more than a year, it is not surprising that their pre-treatment time series behavior is less balanced. Property crimes in particular exhibit very strong trending behavior pre-treatment indicating that curfews are enacted at times when property crimes committed by youth are increasing relative to adults.

Figure 2a shows no evidence of spillover effects on youth just above the curfew age. The figure appears consistent with the hypothesis that the ratio of arrests of youth just above curfew age to those several years older is constant with respect to the time of enactment. Table 4 confirms this impression. Figure 2b indicates that drawing conclusions about spillover effects on arrests for property crimes is more problematic. The strong pre-treatment trend suggests that much older age groups are not suitable controls for youth just above curfew age. Although the upward trend appears to decrease with the imposition of the curfew, we cannot know whether this is indicative of a treatment effect or some more subtle form of mean reversion.

Ultimately the investigation into spillover effects is inconclusive. There is no evidence of important spillovers but the estimates are too imprecise to reject the presence of small effects. If there are any spillovers these results suggest they are most likely quite small. This evidence could be taken to mean that there is little dependence across age-groups in criminal propensity or that any dependence that exists is cancelled out by statistical discrimination on the part of police.

²⁵ The control age-groups are now on average 4 years older than the “treated” age group.

VI. Conclusion

Overall, curfews appear to have important effects on the criminal behavior of youth. The arrest data suggest that being subject to a curfew reduces the number of violent and property crimes committed by juveniles below the curfew age by approximately 10% in the year after enactment, with the effects intensifying substantially in subsequent years for violent crimes.

The magnitude of any biases in the estimates due to spillover effects is difficult to assess. The data do not provide evidence of any spillovers, though given the imprecision of the estimates we also cannot reject modest sized effects. It does seem safe to say that there are probably not any *large* spillover effects, meaning that curfews do not seem to reduce crime in general, but rather only for the targeted age-groups. This suggests that cities designing curfew legislation should choose the statutory curfew age carefully according to which age-groups are in greatest need of intervention.

It is interesting to note that these findings are in keeping with the perceptions of those subject to curfew policies. As Adams (2003) notes “Public opinion shows overwhelming support for curfews... the primary basis for [this] support is the conviction that curfews reduce crime and make the streets safer.” Though this analysis cannot uncover the exact mechanism through which curfews affect crime, the large statutory results suggest youth crime is imperfectly substitutable across time and that temporal targeting of law enforcement policies may be effective.

The lack of noticeable spillover effects has multiple interpretations. One is that police do not statistically discriminate against youth slightly above curfew age. The other is that this

discrimination occurs but is ineffective at reducing crime. A third interpretation is that statistical discrimination occurs and is effective, but its effects are masked by negative dependence across age-groups. The first two stories would suggest that the statutory estimates are truly picking up the deterrent effect of fines. Since the penalties associated with curfew violation are generally small, this claim seems dubious. The third story relies upon a model of crime as a congestible economic activity, a hypothesis which enjoys little empirical or theoretical support.

An alternative rationalization of the evidence is that parents play an important role in the enforcement of curfews over and above that of police. If municipal curfews act as focal points in the establishment of household policies, a curfew with modest fines (and arrests) could lead to large changes in the behavior of youth. The potential role of parents in self-enforcement of curfews is an important area for future research.

The more general policy implications of these findings for criminal justice policy are nuanced. Though curfews appear to be effective at reducing the incidence of crimes committed by juveniles, we have little data on the costs of such programs, either directly in terms of dollars spent enforcing such ordinances, or indirectly in terms of the opportunity costs of policing. Ultimately the desirability of curfew ordinances will be context specific. In cities where a large fraction of youth crime occurs at night and the cost of additional nighttime policing is low, curfews may be an effective law enforcement tool.

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Data Construction

In constructing the data on curfew implementation I aimed to be as conservative as possible. My primary concerns were threefold. First, that the assigned dates of curfew enactment would be too late having been preceded by another enactment a few years prior. Such errors should not only add to the noise of the estimated treatment effects, they bias them towards zero and bias pre-treatment trends away from zero.¹ For this reason I created a detailed legislative paper trail for the curfew policies in each city and an algorithm for categorizing what I had found. The second concern was that the age groups to which the curfew were to apply might be wrong or might have changed over time. I went to great lengths to make sure that the age-groups being compared actually received different treatments, searching legislation and newspaper stories for signs of a change. Again, errors in the assignment should yield treatment effects biased towards zero. Finally, I was concerned that in some cases the passing of curfew ordinances might not be associated with enforcement. There is no perfect way to measure enforcement and I settled upon a simple expedient which was to believe police departments when they said that their curfew policy hadn't changed recently.

Sample Selection and Dates of Enactment

Several sources of information were used to assign dates of curfew enactment to cities. The first is a 1996 survey of police departments with a 1992 population over 100,000 performed by Ruefle and Reynolds (R&R). R&R called every such police department inquiring if a curfew law was in place, when the curfew was originally enacted, and when, if ever, it had been revised or re-enacted. While R&R's survey is most likely a good measure of which cities had curfews in

¹ If curfews are effective and several curfews are enacted in the two periods before they are listed as being enacted, the data will appear to have a negative trend pre-treatment.

1996, its reliability as a measure of when, prior to 1996, a city's curfews were in effect is unknown.² Police departments have a limited institutional memory and it is unclear how they interpreted questions about the date that a city's curfew was "originally" enacted.³

To complement the data found in R&R, I obtained a current copy of the municipal code for every city with a 1990 population⁴ over 180,000. These municipal codes generally contain the initial date of enactment and dates of revision of the sections relating to the curfew. However, in some cases portions of the code have been totally repealed only to be replaced by similar language. In such cases the "initial" date of enactment listed on the code is actually the date of enactment of the most recent curfew language. To avoid this problem, I made heavy use of city municipal clerks, asking them to search for prior ordinances related to juvenile curfew laws. My search was aided by information from newspaper articles which were obtained by searching ProQuest and Lexis-Nexis for articles including the name of the city and the word "curfew." These articles frequently mentioned the history of curfew legislation in the cities in question and the details of their implementation. When an earlier instance of a curfew was found I obtained a copy of the earlier ordinance and in turn searched it for references to earlier laws.

Having assembled a large paper trail for each city, I used the following algorithm to determine the date of enactment. First, I checked for agreement between R&R and other sources over the basic question of whether a city had ever passed a curfew law. Of the 92 cities with 1990 population over 180,000, 77 were listed by R&R as having curfew ordinances. For each of the 15 cities listed as not having curfews, I searched newspaper articles and city codes for

² R&R claim that cities not listed in their table do not have curfews in effect as of February 1996.

³ R&R indicate that in some cases they also contacted the city clerk's office for additional information, but it is unclear how often this occurred or what rule they used in determining whether to call the clerk.

⁴ According to the 1990 Decennial Census.

mention of a curfew law. I found that four of these cities had passed curfews although two of those came after the time of R&R's survey. The two cities with curfews enacted after 1996 were included in the sample of enactors, while the remaining two were dropped under the assumption that a failure to report a curfew to R&R signaled a lack of enforcement.

Next, for the cities that had enacted a curfew according to R&R, I compared the date of enactment found in the city code to the date of enactment in R&R. Because my data spans the years 1984-2000, I first subcategorized the R&R enactment dates by whether they fell into the sample period. According to R&R, 55 of the 77 cities with curfews had enactments or revisions during the sample period. Investigation of the city codes of the 22 that were listed as enacting curfews prior to 1984 indicated that 15 of them in fact had revisions during the sample period that were not listed in R&R. Nevertheless, I dropped all 22 cities from my analysis under the assumption that the failure on the part of the police department to recall recent revisions/re-enactments was an indicator of lax enforcement.⁵

For cities that were listed in R&R as enacting a curfew during the sample period, I used the following rule to assign enactment dates. If the city code listed a date in the sample period that was identical or prior to R&R I used the earliest such date. If the R&R date was prior to the earliest code date in the sample period, I called the city clerk to search for earlier ordinances. In most cases I was able to find prior ordinances with enactment dates identical or prior to those found in R&R. Of the 55 cities that R&R listed as enacting curfews during the sample period, the assignment rule yielded a date that agreed with R&R in 40 cases. Of the 15 remaining cases 12

⁵ In regressions available from the author I have experimented with including the 15 cities with revisions/re-enactments during the sample period in the analysis. The results are substantively unchanged.

yielded enactment dates prior to those found in R&R while the remaining 3 had enactment dates after those found in R&R.

Inspection of newspaper articles indicated that 3 cities (Baltimore, Miami, and Dallas) had injunctions issued against their curfew ordinances preventing them from being enforced at the time of enactment. All of these cities eventually won their court battles and thus I changed the date of enactment to reflect the post-injunction date of enforcement. A fourth city, the District of Columbia, also faced an injunction against its curfew ordinance, which was subsequently declared unconstitutional by the courts. Since DC attempted to enact curfew laws several times, each time suffering a defeat in court, I drop it from the sample.

Finally, two cities had exceptional circumstances that warranted dropping them from the sample. The first, Hialeah, according to the city attorney, never enforced a curfew, but was forced to legally adopt one as part of a measure passed by Dade County. The second, Las Vegas, was dropped because its only curfew enacted during the sample period applied to “high school students” without specifying a particular age range and only covered a narrow geographic area.

The final sample contained 54 cities enacting curfews (55 enactors – 3 exceptions + 2 enactors not listed in R&R). For each city, I rounded the date of enactment to the next year if the curfew was enacted in December or November.⁶ I also included in the sample the 11 cities that never enacted curfews according to both R&R and inspection of city codes. These cities were used as controls for purposes of estimating year effects. That makes a grand total of 65 cities.

The following table summarizes the major decisions leading to cities being dropped:

⁶ Inspection of newspaper stories indicate that it is common for curfew ordinances enacted in November and December to begin enforcement starting on January 1st of the following year.

Summary of Sample Selection Criteria

Cities with population $\geq 180,000$	92
Cities with curfews enacted during sample period but not listed in R&R	-2
Cities listed in R&R as having curfews not enacted or modified during the sample period	-22
Exceptional cities (DC, Hialeah, Las Vegas)	-3
Final Sample Size	65

Maximum Curfew Age

In all cases the maximum statutory curfew age was taken from the city code or relevant ordinance. Among the 54 cities listed in R&R as enacting curfews during the sample period, 46 had information identical to that found in their city code/ordinances. Two of the eight cities with different age information had different enactment dates. The remaining six discrepancies appear to be mistakes on the part of R&R.

Table 1: Curfew Data by City

City	State	Population in 1990	Year Curfew Enacted	Statutory Curfew Age
Akron	OH	223,019	1990	17
Albuquerque	NM	384,736	1994	16
Anaheim	CA	266,406	1990	16
Anchorage	AK	226,338	1989	15
Arlington	TX	261,763	none	N/A
Atlanta	GA	394,017	1991	16
Austin	TX	465,577	1992	16
Baltimore	MD	736,014	1995	16
Baton Rouge	LA	219,531	1995	16
Birmingham	AL	265,852	1996	16
Boston	MA	574,283	none	N/A
Buffalo	NY	328,123	1994	16
Charlotte	NC	396,003	1985	15
Cincinnati	OH	364,040	1994	17
Cleveland	OH	505,616	1993	17
Colorado Springs	CO	281,140	1992	17
Corpus Christi	TX	257,453	1991	16
Dallas	TX	1,006,831	1994	16
Denver	CO	467,610	1994	17
Des Moines	IA	193,187	none	N/A
Detroit	MI	1,027,974	1987	17
El Paso	TX	515,342	1992	16
Fort Worth	TX	447,619	1994	16
Fresno	CA	354,202	1990	17
Garland	TX	180,635	1994	16
Glendale	CA	180,038	1989	17
Greensboro	NC	183,521	none	N/A
Houston	TX	1,630,672	1992	17
Jackson	MS	196,594	1992	17
Jacksonville	FL	635,230	1991	17
Jersey City	NJ	228,537	1987	16
Kansas City	MO	435,141	1991	17
Lexington-Fayette	KY	225,366	1995	17
Lincoln	NE	191,972	none	N/A
Long Beach	CA	429,433	1988	17
Los Angeles	CA	3,485,398	1988	17
Louisville	KY	269,157	1997	17
Lubbock	TX	186,281	1994	16
Madison	WI	191,262	1992	16
Mesa	AZ	288,091	1991	17
Miami	FL	358,548	1996	16
Mobile	AL	196,278	2002	17
New Orleans	LA	496,938	1994	16
New York	NY	7,322,564	none	N/A
Newark	NJ	275,221	1993	17
Norfolk	VA	261,229	1993	17
Oklahoma City	OK	444,730	1994	17
Omaha	NE	335,795	none	N/A
Phoenix	AZ	983,403	1993	17
Raleigh	NC	207,951	none	N/A
Richmond	VA	203,056	1992	17
Rochester	NY	231,636	none	N/A
Sacramento	CA	369,365	1995	17
San Antonio	TX	935,927	1991	16
San Diego	CA	1,110,549	1994	17
San Jose	CA	782,225	1994	17
Seattle	WA	516,259	none	N/A
Shreveport	LA	198,528	1992	16
St. Paul	MN	272,235	1990	17
St. Petersburg	FL	238,629	none	N/A
Tampa	FL	280,015	1994	16
Toledo	OH	332,943	1993	17
Tulsa	OK	367,193	1995	17
Virginia Beach	VA	393,069	1989	17
Wichita	KS	304,011	1992	17

Table 2: Summary Statistics¹

Outcome	Age-Group ²		
	1	2	3
Log Arrests for Violent Crime	3.815 (1.431)	3.943 (1.359)	3.790 (1.271)
Log Arrests for Property Crime	5.126 (0.975)	5.068 (0.904)	4.534 (0.877)

¹ First number in each box is the mean of the variable, number in parentheses is standard deviation.

² Actual ages contained in each age-group vary by city. Age group 1 is the treatment group for the statutory estimates, age-group 2 is the control group for the statutory estimates, and age-group 3 is the control group for the spillover estimates. See Tables 3 and 4 for exact age compositions by outcome.

Table 3: Statutory Treatment Effects

Outcome	Treatment Group Age Composition	Control Group Age Composition	Estimator	Restriction	Contemporaneous	Short Run	Long Run	Average
Violent Crimes	curfew age	curfew age+1	LAD	$\beta_{-1} = 0$	-0.082 [0.110]	-0.208 [0.012]	-0.306 [0.024]	-0.232 [0.014]
				$\beta_t = 0 \forall t < 0$	-0.081 [0.064]	-0.155 [0.010]	-0.284 [0.010]	-0.228 [0.006]
OLS			$\beta_{-1} = 0$	-0.051 [0.346]	-0.126 [0.106]	-0.209 [0.100]	-0.151 [0.086]	
			$\beta_t = 0 \forall t < 0$	-0.060 [0.290]	-0.150 [0.094]	-0.248 [0.068]	-0.179 [0.050]	
Property Crimes	curfew age	curfew age+1	LAD	$\beta_{-1} = 0$	-0.077 [0.038]	-0.092 [0.276]	-0.154 [0.180]	-0.116 [0.184]
				$\beta_t = 0 \forall t < 0$	-0.082 [0.036]	-0.109 [0.056]	-0.186 [0.066]	-0.153 [0.058]
OLS			$\beta_{-1} = 0$	-0.059 [0.100]	-0.083 [0.088]	-0.205 [0.102]	-0.131 [0.062]	
			$\beta_t = 0 \forall t < 0$	-0.068 [0.062]	-0.105 [0.024]	-0.242 [0.026]	-0.158 [0.014]	

Estimates taken from specification of form given in equation (2).

Numbers in brackets are bootstrapped p-values

Short run effects refer to the average of the coefficients in periods t=1,2, and 3.

Long run effects refer to the average of the coefficients in periods t=4,5, and 6.

Average effect refers to the average of coefficients in periods 0 through 6.

Table 4: Spillover Treatment Effects

Outcome	Treatment Group Age Composition	Control Group Age Composition	Estimator	Restriction	Contemporaneous	Short Run	Long Run	Average
Violent Crimes	curfew age+1	curfew age+4,5,6	LAD	$\beta_{-1} = 0$	-0.001 [0.996]	-0.012 [0.882]	-0.028 [0.978]	-0.017 [0.960]
				$\beta_t = 0 \forall t < 0$	0.014 [0.776]	-0.005 [0.800]	-0.024 [0.980]	-0.011 [0.862]
OLS			$\beta_{-1} = 0$	-0.018 [0.734]	-0.030 [0.634]	0.055 [0.602]	0.008 [0.866]	
			$\beta_t = 0 \forall t < 0$	-0.026 [0.704]	-0.051 [0.470]	0.022 [0.824]	-0.016 [0.866]	
Property Crimes	curfew age+1	curfew age+4,5,6	LAD	$\beta_{-1} = 0$	-0.007 [0.666]	0.014 [0.832]	0.053 [0.550]	0.028 [0.630]
				$\beta_t = 0 \forall t < 0$	-0.018 [0.338]	-0.020 [0.748]	-0.030 [0.738]	-0.024 [0.724]
OLS			$\beta_{-1} = 0$	-0.015 [0.676]	0.050 [0.196]	0.175 [0.122]	0.094 [0.124]	
			$\beta_t = 0 \forall t < 0$	-0.026 [0.508]	0.020 [0.538]	0.126 [0.206]	0.059 [0.240]	

Estimates taken from specification of form given in equation (2).

Numbers in brackets are bootstrapped p-values

Short run effects refer to the average of the coefficients in periods $t=1,2$, and 3.

Long run effects refer to the average of the coefficients in periods $t=4,5$, and 6.

Average effect refers to the average of coefficients in periods 0 through 6.

Figure 1a: Dynamic Effect of Curfew Enactment on Arrests of Youth Just Below Curfew Age for Violent Crimes

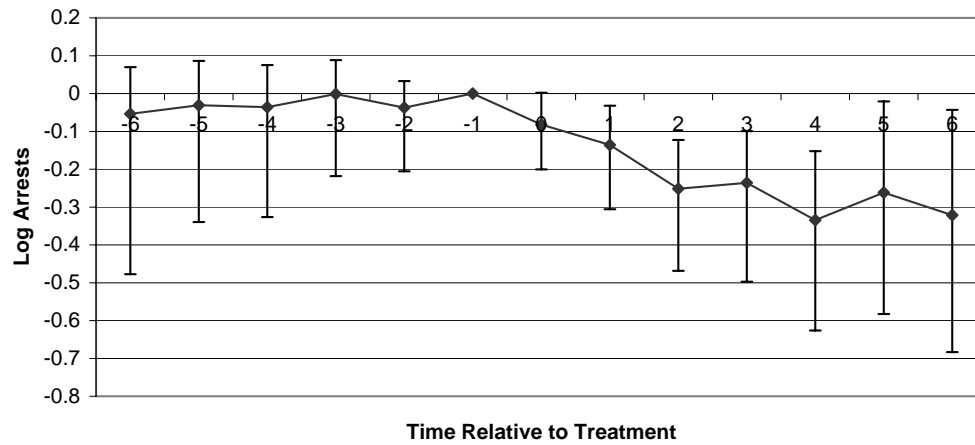


Figure 1b: Dynamic Effect of Curfew Enactment on Arrests of Youth Just Below Curfew Age for Property Crimes

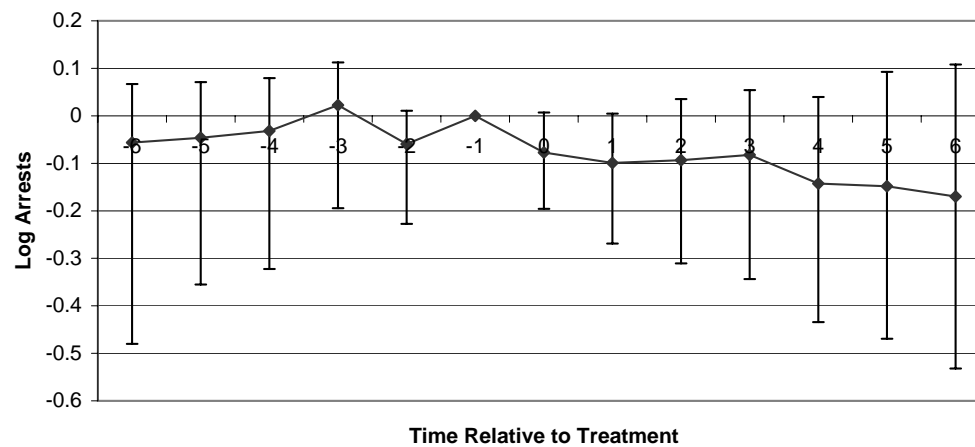


Figure 2a: Dynamic Effect of Curfew Enactment on Arrests of Youth Just Above Curfew Age for Violent Crimes

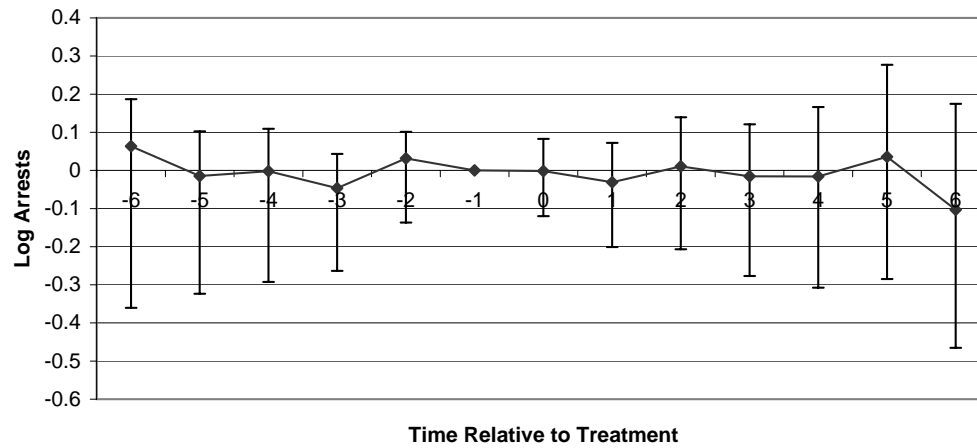


Figure 2b: Dynamic Effect of Curfew Enactment on Arrests of Youth Just Above Curfew Age for Property Crimes

